method of designation in a system of symbols such as other sciences have found? I am not in sympathy with those who look for relief in a laxer more open administration of the present system. Such a line of action does not seem to me likely to prove either effective or legitimate.

This rigor in systematic nomenclature is a natural reaction from the free and easy methods which have prevailed in the past. Biological science even to-day publishes loose, inaccurate statements in research contributions which would be laughed out of court in physics or chemistry, to say nothing of mathematics or astronomy. It is necessary that some reform be undertaken, that our branch of science approach more closely to the precision in observation and experiment, in record and discussion that characterizes older sciences. The natural lack of fixity in biological phenomena has been utilized to excuse a lack of precision in method and investigation which must be corrected. One effort to reach a more justifiable basis is seen in the recent development of statistical work, and in the publication of definite numerical results rather than merely generalizations in connection with experimental work, in the effort to control more accurately and state more precisely the conditions of such experimentation and to analyze more closely the results obtained. In such lines zoology has achieved wonderful progress in the last twenty years or even less.

The same influences will lead to a reform of our system and, following the lead of other sciences, such a reform is likely to be accompanied by the simplification which is associated with the utilization of symbols. The far-seeing biologist should be on the watch for a plan which promises some measure of success in this line, he should welcome all reasonable attempts at the solution of the problem. Of course he will not reject any and all systems because they are new departures; and yet he should not fail to subject each to careful consideration because it may seem to be inadequate or only partially worked out. Out of such careful discussion will come the longed-for result in a workable form. But the system itself will represent contributions from many sources.

I confess that Professor Needham's plan seems to me at most only a partial solution of the problem. Even as such it may prove to be of great value and it is to be hoped that biological workers may be willing to try it on various groups in diverse portions of the field and may then report on their results. Better still if it could be subjected to a trial by some recognized society or institution with a view to testing thoroughly its character. It would be valuable to compare it carefully with the much more complex system proposed some years ago by Tornier[4] which seems to have attracted no attention, although it was a most ingenious and original means of formulating a symbolism for zoological nomenclature. While this system was much more complex and covered not only species as proposed by Professor Needham's plan, but also genera, and indicated the precise place in class, order and family occupied by each genus and species, yet apparently the symbol used for a given form would not be permanent and independent of changing views regarding the position and relationship of genera and higher groups. This lack of stability would be a serious, if not fatal, objection to the introduction of a new system planned to correct precisely the same defect in the old.

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## BOTANICAL EVIDENCE OF COASTAL SUBSIDENCE

IN a recent article[1] Professor D. W. Johnson calls attention to certain conditions at Scituate, Mass., which are there responsible for a fictitious appearance of coastal subsidence. During the "Portland Storm" of 1898 the bar was broken which at that locality almost separates the North River marshes and bay from the ocean, with the result that the high tide level on the marshes is now from one to several feet higher than it was then.

[4] *Zool. Anz.*, Vol. 21, p. 575, October 24, 1898.

[1] SCIENCE, N. S., XXXII., 1910, p. 721.

Trees surrounding the marsh have been killed and are now standing among salt marsh grasses. Since breaches in bars, widening of tidal inlets and other shore-line changes may cause local fluctuations of a number of feet in the high-tide surface, Professor Johnson hastily concludes that no evidence of recent subsidence on the New England coast thus far presented can be considered satisfactory.

It is the mean sea level, rather than the irregular and changeable high-tide surface, which is most important in discussions of coastal subsidence. Below mean sea level the salt-marsh builders, *Spartina glabra* and *Spartina patens,* can not live. As a matter of fact *Spartina patens,* by far the more important of the two plants, seems to occur in significant amount only within a vertical range of about two feet. It builds its turf up to mean high-tide level. Above this level it is for the most part replaced by other plants, notably by *Juncus Gerardi* and various species of *Scirpus.* We may, therefore, regard any thickness of *Spartina patens* turf greater than two feet as a measure of change in high-tide level. If such turf extends to mean sea level, or below it, the evidence that subsidence has taken place is indubitable. If a deep turf is of uniform texture throughout, a strong presumption is created that subsidence has continued uniformly to the present time. *Spartina patens* turf lies below mean sea level at many places in the vicinity of Boston, as, for example, near the mouth of the Saugus River, where it forms a homogeneous stratum to a depth more than ten feet below high-tide level. The geological significance of turf formation by *Spartina patens* has been pointed out by Professor C. A. Davis,[2] of the Bureau of Mines. He presents substantial evidence that coastal subsidence is now going on—evidence to which the hypothesis of a fluctuating high-tide level has no possible application.

Aside from the value of the salt marsh deposits themselves as indicating subsidence, there is still another type of botanical evidence which Professor Johnson should not so lightly have disregarded. In certain cases where fresh water peat is found below mean sea level, in obviously undisturbed relationship to the substratum, whether or not the deposits have ever been invaded by the sea, we have incontrovertible evidence of subsidence. The full argument in support of this statement has been published elsewhere,[3] but may be briefly reviewed here.

Peat deposits are of two main types, depending upon the relation of the water table to the ground surface in the depression in which they are formed. In the first type the depression contains a pond or other body of water, so that the peat is derived from aquatic plants and from the surface of the water, through the agency of mat-forming plants. If the water in such a depression were fresh, a mat might be formed at or slightly above sea level, and fresh-water peat of this type would then be laid down below sea level. If the ocean should break into such a bog it is conceivable that the mat might settle considerably and then be covered by salt-marsh deposits. Such a condition I have never seen.

The second type of peat deposit is built up from the ground by successive elevations of the water table, as we know from the character of the plant remains which it contains. At every stage of growth its surface has very nearly coincided with the ground water level. Since in coastal Massachusetts the water table is never lower than sea level, a bog of this type, if its bottom is lower than mean sea level, must of necessity be interpreted as a record of subsidence.

There is a locality at Quamquisset Harbor, on the coast of Buzzard's Bay, where the sea has cut into a *Chamæcyparis* bog. At the water's edge a sounding showed uniform peat, containing *Chamæcyparis* wood, to a depth of fourteen feet below mean sea level. The sand bottom of the deposit had not been disturbed by under-cutting. At the point where this sounding was made, there were salt-marsh deposits in a thin layer overlying the peat,

[2] "Salt Marsh Formation near Boston and its Geological Significance," *Economic Geology,* V., 1910, p. 623.

[3] "The Submarine Chamæcyparis Bog at Woods Hole, Mass.," *Rhodora,* XI., 1909, p. 221.

but further inland the *Chamæcyparis-Sphagnum* peat was still forming, at an elevation perhaps three feet above high-tide level, and soundings showed that the deposit was uniform from top to bottom. Within a very few feet of the seaward edge of the marsh there are still two or three small stumps which project several inches above the *Spartina patens* turf which has grown up around them. These constitute a strong argument that subsidence is still going on. If there had been no subsidence for 3,000 years, as Professor Johnson thinks likely,[4] these little stumps would surely have rotted away by this time!

The Coast and Geodetic Survey has furnished data regarding the tides in Quamquisset Harbor. Spring high water is 2.4 feet above mean sea level. Mean high water is 2.0 feet above mean sea level. The highest tides observed were 3.0 feet above mean sea level. These tides at Quamquisset are so low that Professor Johnson's hypothesis of a fluctuating high-tide level can not possibly be invoked in explanation of the submarine peat beds.

After examining one locality where salt-marsh plants have invaded a fresh-water vegetation under conditions certainly far from typical, and where all of the fresh-water remains are found at extreme high tide, Professor Johnson has ventured to characterize all the evidence which has been offered for recent subsidence as inconclusive. As a matter of fact his hypothesis has no bearing whatever on most of the evidence which has been offered.

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## FACULTY OR PRESIDENT?

THE discussion of the merits of control by the faculty or by the president in any educational institution, which has been presented from time to time in SCIENCE, was continued by some references in Professor Cattell's article in the issue for November 11, and by a short paragraph in the abstract of President Schurman's annual report in the following number. There is still another angle from which the matter may be viewed.

The student body in a college, or university, is a comparatively constant quantity. The great majority of students spend the four years from about eighteen to twenty-two in the institution of their choice. The average age of the student body, taken as a whole, would, therefore, be slightly under twenty, owing to the somewhat greater numbers in the freshman and sophomore classes. Whatever fluctuations there might be from year to year, in consequence of an exceptionally large or surprisingly small entering class, or because an unusually large number from the upper classes turned to professional work before graduation, they would be within very narrow limits, so narrow, indeed, that the entire body of students might be regarded as an individual not quite twenty or just over twenty years of age. The same would be true of a university with the various professional schools and the liberal arts or undergraduate department, although the average would probably be three or four years higher. The student body itself, none the less, would be comparatively stable.

If we turn our attention to the faculty, we find another fairly constant quantity. Since the retiring age is somewhere about sixty-five or seventy—it might be a little over or a little under—and since the youngest instructors are just out of college, the average age of a faculty would be somewhere between forty and forty-five, as a rough estimate. If there should be an unusually large number of young instructors, or an extra large number of elderly professors, then the average age would be lowered or raised correspondingly, but in either case it would not be far from the age mentioned above, and from year to year the fluctuations would be within rather narrow limits, so that there would be a fairly stable body to exercise control of whatever sort. In those institutions in which the youngest instructors have practically no voice in the administration, the average age would be raised, but the faculty would retain its characteristic of a constant quantity.

When the problem is presented in this way,

[4] SCIENCE, N. S., XXXII., 1910, p. 709.